felt, and as Mr. Swinburne points out, the attitude of the Royal Society is of prime importance. Many would regret if the Society to which the "Principia" was communicated ceased to publish physical work, and indeed if we know anything of the feelings of English Physicists, we do not think that such a catastrophe is probable. On the other hand, it is impossible not to recognise the fact that the Royal Society is an obstacle to the realisation of a satisfactory scheme for the publication of English physical papers.

The Physical Society was founded because at that time the Royal Society offered no facilities for the experimental illustration of communications made to it. The meetings of both the elder and the younger society are fully occupied with the work now undertaken, in spite of the fact that the discussions at the meetings of the Royal Society are short.

If to-morrow all English physicists were to agree to send all their work to the Royal Society there would not be time to discuss it, and many of the papers thus offered, though worthy of publication, would be regarded as not of the type which the Society affects. Yet if there is to be organisation, if order is to succeed chaos, it can only be either by a friendly struggle between the Royal and the Physical Societies, which would not be likely to lead to any definite result at present, or by still more friendly co-operation between them, by which all that is desired might be attained in a few months. That going forward or standing still are alike difficult is undeniable. It is obvious that the conditions which apply to physics apply to other branches of natural knowledge. We shall be glad if those most closely interested will try to smooth the way by discussion in our columns.

THE CAUSES OF GLACIAL PHENOMENA.

The Glacial Nightmare and the Flood: a Second Appeal to Common Sense from the Extravagance of some Recent Geology. By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S., &c., &c. (London: Sampson Low, Marston and Co., 1893.)

IT is not uncommon to find that men who have devoted much time and careful research to the elucidation of complex phenomena have experienced all the phases of thought through which a succession of previous observers have passed in bringing the subject to its then present stage. This is more usual in certain classes of inquiry than in others, and in such it is clearly helpful to dwell upon the history of the development of opinion upon the question. It is giving, as it were, the embryology of an idea in order to enable the reader to understand better the adult form. In the volumes before us Sir Henry Howorth has rendered this good service to students of glacial phenomena.

The title of the book is unfortunate and may prejudice many against what is really a scientific work of great value.

Sir Henry first gives a sketch of the views of the earlier writers who referred all the phenomena in question to the action of water; then he explains how by degrees the agency of icebergs was called in; how it was next considered that larger glaciers would account for most of the facts; and how, after that, it was supposed that they were to be explained only on the hypothesis of great ice-sheets extending south from either pole, even to the tropics according to some.

These ice-sheets must, of course, be accounted for by exceptional agencies, such as secularly-recurring astronomical combinations, in connection with which the author discusses the obvious inference that there must have been similar combinations and similar results in previous ages. He then devotes almost the whole of another volume to the various incidental theories which have grown up round the theory of circumpolar glaciation, or are necessary to it, and, finally, admitting a moderate extension of glaciers, he sums up in favour of stronger and more widely extended marine action than has of late been generally admitted.

As we read we cannot but learn to admire the shrewd observation of the older geologists, though the true explanation of the phenomena had not yet been put forward. We see how the obvious suggestion that great rushes of water would account for it all, set Von Buch and Hopkins to calculate what depth and velocity of water would be required to obtain force necessary to transport the blocks perched on the hills; and if there were difficult cases which made some of the "Champions of Water," such as Mierotto, De Luc, and Hall call in the aid of icebergs, still there was the fact that a great deal of the drift appeared to be sorted by water, and that, in great floods, boulders several feet in diameter have been hurried along the rocky bed of a stream with a noise like thunder; that large stones are often tossed by the storm waves to the top of precipices on our western rock-bound coast, as may be seen on a smaller scale where single stones are thrown on to a pier or promenade, though the sea-wall may be almost vertical. The gravel carried by a spate over the meadows is just like that found in the Esgairs, and is thrown up on either side of the torrent in long ridges. There is no doubt that a great deal of what is included in the drift, especially in Germany, is just like what is carried by flood water. It would not be comfortable to feel that the great old heroes of geology advocated views impossible in physics and unsupported by observation. Whether better explanations may in many cases now be offered is another question.

When it was once admitted that the glaciers were formerly much more extensive, and the drift round mountain regions was referred to their agency, it is easy to see how the impossibility of accounting for the occurrence of glaciers in North Germany, where what was thought to be similar drift was widely spread, led to speculation as to the possible extension of ice-sheets from high latitudes all over north-western Europe and north-eastern America, and the views of Bernhardi and Schimper, which involved an ice-sheet coextensive with the distribution of the drift began to be received with favour.

After this was given up as the *direct* cause, it was still held that its *indirect* effects would be very potent in producing extremes of climate alternately in the north and south hemispheres. The question now naturally arose whether there were any recurring glacial conditions in past times, and evidence of such action was seen in rounded surfaces and striated stones from various ancient rocks.

But most of the examples were from localities where the included fragments had been crushed against one another by earth movements, the grooves running alike across the included pebbles and the matrix in which they were imbedded. Or they were from the neighbourhood of what had been mountain ranges repeatedly through long ages. Even if we admit that some ancient conglomerates appear to have derived their boulders from glacial debris, that does not make the conglomerates glacial, but only requires glaciers in the adjacent mountains then as now. On the hypothesis of the geographical origin of glacial conditions, seeing that there must have been elevations and submergences over and over again, glacial phenomena should recur near the areas of upheaval, only without that periodicity which is required by the astronomical theory. There is, moreover, in the fossil flora and fauna no evidence of the recurrence of widespread glaciation such as would justify our referring it to a glacial epoch.

The astronomical glacialists further hold that not only were there secularly alternating periods of cold and heat in either hemisphere throughout the ages, but that within each period there were shorter periods of greater and less intensity of cold, of which we find evidence in the so-called interglacial beds of Britain, and in such deposits as those of Dürnten, where glacially striated pebbles underlie lignite which is covered also by morainic debris. But all advocates of the geographical explanation suppose that the earth movements on which it depends were discontinuous and subject to considerable oscillation, while the advance and retreat of glaciers, as a mere weather result, is so marked that we may safely admit that, as a climatic result of oscillations of level, it might be quite as great as required by any of the cases referred to.

Croll says that by far the most important of all the agencies, and the one which mainly brought about the glacial epoch, was the deflection of ocean currents, but he does not show that it is not possible to account for this deflection by earth movements.

There is one very important fact which does not seem to be generally recognised, namely, that the last glacial conditions extended only over a limited area on either side of the North Atlantic, and that this limitation must be referred to geographical causes, so that, if these were sufficiently powerful to determine the area, they may account for the glaciation itself. "What is wanted, however," our author remarks, "is not testimony to sporadic glaciers or local ice action, but to widespread glacial phenomena such as would witness an ice age."

The absurdity of the answer that percolating water must have removed ice markings from the surface of the stones is sufficiently obvious to any one who has had his attention called to the much finer markings on fossils, &c., which have been preserved.

The grooved stones of Devonian Age in Victoria are worth about as much as the facetted stones from Gorplitz by Barna, which are supposed to prove the great southerly extension of glacial action in Germany, but which more probably owe their form and condition to blown sand.

Several ingenious explanations have been offered of the occurrence of marine shells in stratified drift on the high ground of southern Sweden and northern England and Wales. The more obvious explanation is, of course, that they were left there as the shingly shore of the receding post-glacial sea. But this would have involved earth movements in comparatively recent times to so great an extent as would lend probability to the theory of such elevations as would account for glaciers in temperate regions, and of such submergences as would explain the widespread post-glacial sands and gravels. Some therefore suggested that these masses had been scraped up from the sea bottom and been pushed up the mountains to their present position; that they were, in fact, part of a great terminal moraine of the polar ice-sheet. Some got over the difficulty of explaining the even stratification and the ripple marks on the beds, as well as the non-Arctic character of the shells, by supposing that the sand and shells were pushed up in the ice from the sea bed in temperate regions, but that the deposit in which they are now seen was washed from the ice-foot at these several elevations by the fresh water due to the melting of the ice, and bearing away with it the mud, sand, and stones transported so far in the ice.

Too much stress must not be laid upon stratification and lines of boulders in the drift, as this may be produced by iceberg loads being thrown down in deep water; just as when a handful of mixed sand and grit of various form and different coarseness is dropped into a long glass of water, the larger grains will, *cœteris paribus*, reach the bottom first, and a rough stratification will be produced. The contortion of clays and sands can often be explained by the loads of debris carried by icebergs and dropped upon them, squeezing the underlying plastic mass away, and rolling up the surrounding layers in every variety of fold and crumple.

Our author lays great stress on the fact that there is now no polar ice-cap at all, and that all the evidence shows that the pole is not, and never has been, the centre of greatest cold or of greatest glaciation. The ice radiated from Scandinavia, not from the pole, and the pillars and prominent unglaciated rocks of Northern Asia show that there has been no ice sheet there in recent ages.

In his advocacy of a considerable submergence in comparatively recent times our author has the support of Prof. Prestwich, who, in the last number of the Proceedings of the Royal Society, has expressed the opinion that the masses of unstratified rubble commonly found resting on the slopes and terraces along the English Channel seem to be due to a force of propulsion for which the hitherto generally-suggested causes are manifestly inadequate. He extends his generalisation over Western Europe and the coasts of the Mediterranean, and arrives at the conclusion that the loess was a sediment deposited from the turbid sea-waters during the submergence, while the superficial deposits called "head" he refers to the surface debris swept off by divergent currents during the subsequent upheaval. Both of these movements he refers to periods of such short duration that large numbers of animals were simultaneously drowned and the waters were rendered so turbid as to be unsuited for marine life.

Our author explains many of the phenomena of the later drift by reference to a great submergence, but, wishing apparently to imply that it was of a still more transient nature, speaks of it as a great flood. His flood deposits are not, however, the same as those referred to by Prof. Prestwich.

Sir Robert Ball has pointed out that if the heat received in winter is distributed over thirty-three more days, instead of only over seven more, the result would be glacial conditions in the northern hemisphere, a result which has been somewhat modified by Mr. Hobson, who pointed out that the heat received over the regions within the Arctic circle should be omitted from the calculation.

But the opponents of the astronomical theory are prepared to admit that when we are dealing with operations in which the effects are so obviously cumulative and the reaction of one on the other so important, we may expect in climate, as there are in what we call weather, times of such unstable equilibrium that, for instance, a slight prolongation of the period of cold, which may be small in itself, may yet cause a local change, the effects of which may eventually become very far-reaching—as, in the case of weather, rain may be produced by the explosion of a small quantity of dynamite in the upper regions of the atmosphere. It is useless to deny the existence of such causes as those on which the advocates of the astronomical theory chiefly rely any more than we should deny the possibility of the detection of tidal action in one of the American lakes, because we are convinced that the real cause of the rise of six feet or so of water on one side is due to the gale which we observe blowing across the lake. Nor are we justified in rejecting the possibility of more or less rapid submergences resulting in a rearrangement of surface debris or even in more cataclysmic action of the same kind, as was seen in the earthquake wave that rolled in on Lisbon.

We do seem to require some simpler theory than that of the extreme glacialists to explain the phenomena of the Pleistocene Age.

If the north-eastern portion of America and the north western part of Europe were raised so as to get a snowy mountain range on either side of the Atlantic, sending ice-sheets down to the sea in the intervening depressed trough, and by the convergence of the axes of elevation deflecting the ocean currents and causing glaciers to creep down east and west from the mountain ranges—all the phenomena of the glacial epoch could be explained.

Reverse the process; send up Greenland and lower the North American and Scandinavian chains even to below where they now stand, bringing in again the warm currents from the South, and the post-glacial submergence takes its place. Let the Icelandic volcanic system play its part, and let there be earthquakes and jerks and oscillations, all part of the regular course of operations accompanying such movements, and we have the marine drifts all explained. The forms of life which have been driven away from the centres of ice dispersal will follow the receding glaciers back again. Observers will find in their own district evidence of land ice, or of icebergs, or of sea currents, or of glacier water, but in this less cumbrous theory there will be room for all.

The conflict of views recorded in Sir Henry Howorth's exhaustive work prepares one to believe that the matter may not be finally settled for some time, and, before public opinion comes to rest, we may expect many swings of the pendulum now far on this, now far on that side of the truth. But we welcome this protest against the extravagant views of the extreme glacialists and this valuable encyclopædia of the facts and arguments bearing on glacial phenomena which must be in the hands of every student of the subject. Our author is well known for his scientific treatment of literary subjects and for the literary skill with which he presents his scientific facts. Though he is an uncompromising advocate of what commends itself to him as the right view, he has indulged in no criticism which can be regarded as discourteous to the living or unfair to the dead.

T. MCKENNY HUGHES.

DYNAMO-ELECTRIC MACHINERY.

Original Papers on Dynamo Machinery and Allied Subjects. By J. Hopkinson, M.A., D.Sc., F.R.S. (New York: The W. J. Johnston Company, Limited. London: Whittaker and Co., 1892.)

The Dynamo. By C. C. Hawkins, M.A., A.I.E.E., and F. Wallis, A.I.E.E. (London: Whittaker and Co., 1893.)

THERE is hardly any greater authority on the subject of dynamo-electric machinery than Dr. Hopkinson. It was he who, turning his attention to the Edison machine, first showed how the iron in the magnets should be distributed, how the magnetising coils should be wound, and the machine built up so as to ensure its possessing the highest possible efficiency in every sense of the word.

This he did not attempt to do by mere theoretical speculation, though himself a great theorist, but by instituting a very complete and exhaustive set of experiments on dynamo machines under practical conditions, and graphically representing their results. No device in the whole history of the evolution of the dynamo has been of more general service than his plan of exhibiting the results of experiments in the well-named characteristic curve of the machine. This did for the dynamo what the indicator diagram had long been doing for the steam engine, though not, of course, in the same way.

With the most admirable simplicity this curve of electromotive forces as ordinates, and currents as abscissæ, gave just the information required regarding the action of the machine. Thus, when the ordinates represented the potential differences between the terminals, the inclination to the axis of abscissæ of the line joining the origin to any point gave the working resistance in the external circuit, corresponding to the current and potential difference defining the point to which the line was drawn, or, this resistance being known, gave the current and potential difference which the machine might be expected to develop with this as the working part of the circuit.

Then again, Dr. Hopkinson showed how the characteristic curve could be used to give the conditions under which an arc lamp can be made to work. It is well known that if the generating machine working on an arc lamp be run so as to give an electromotive force below a certain limiting value, the machine cannot be made to "keep" an arc. An explanation had been previously given by Dr. Siemens; but Dr. Hopkinson showed that all that was necessary was to lay down in the characteristic curve of the dynamo as already explained the line